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## FACT AND FANCY IN THEORIES CONCERNING ACTS— *Concluded*

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One consideration presented by Professor Jackson in his letter containing the first draft of his criticism, mentioned above, led me to alter a view which I had expressed in my *Composition and Date of Acts*. In discussing (*ibid.*, pp. 18 f.) the passage Acts 8:10, I employed as one argument the supposed fact that the city Samaria (Sebaste) was peopled by pagans, and that few, at all events, of the Samaritan *sect* were to be found there. Professor Jackson remarked very justly that in that case I Acts would here contradict its otherwise consistent representation, that the first evangelization of the Gentiles was that by Peter. I accordingly went carefully through the available material bearing on the population of the city Samaria, and satisfied myself completely that the view which I had expressed was without satisfactory foundation, and that Wellhausen and those who, like myself, have accepted his conclusion without sufficient study of the reasons for it, have been too hasty. The city had not only a strong pagan element in its population, but was also (as we should expect *a priori* for every reason) one of the chief cities of the Samaritan sect.<sup>1</sup> The evangelization described in Acts 8: 4-25 was confined to this sect, as seems in fact to be indicated by the wording of the passage; note vs. 9, where it is after all not easy to explain away ἔθνος as a translator's error. Vs. 25, "many villages of the Samaritans," gives further evidence. In all probability, therefore, the carrying of the gospel to the Samaritan sect is intended in 1:8 (why otherwise should Samaria be specified?). The self-consistency of I Acts is thus maintained here as elsewhere. I wrote at once (December, 1916) to Professor Jackson, correcting my former

<sup>1</sup>I hope to discuss the evidence of this in some other place, when I have opportunity.

statement. I should add that the conclusion reached in my investigation of the translation-Greek of the passage 8:10, that the meaning originally intended was "This is the power of the God who is called Great," seems to me still the only probable one.

The whole question of the unity of I Acts has been ably discussed, with an affirmative conclusion, by Professor Wilson in his article entitled "The Unity of the Aramaic Acts." I need not say that I have been deeply interested in his arguments and gratified by his support. I too have become more and more firmly convinced, in my continued study of I Acts, that it is not only an organic and harmonious whole but also carefully proportioned, and a work of high literary merit in this, that it chooses from the mass of material which must have been available only such things as were truly typical. Nothing which it furnishes is trivial, everything is significant. One of the most important qualities of a historian is the instinct which enables him *to select*, and this quality at least the Judean narrator has in high degree. His work is more carefully planned than II Acts. The latter, mainly because of its very nature, is loosely put together and somewhat casual, with all the literary taste and skill which it shows. Wilson demonstrates with success (pp. 325-27, 330-32) how certain characteristic motives run, like colored threads, through the whole texture of the Aramaic document, from chapter 1 to chapter 15. The only place where he has found evidence of more than one stratum in the narrative is in chapters 4 and 5:17-42, where the one account has seemed to him to duplicate the other. This judgment of his was expressed in a former article of his, "Some Observations on the Aramaic Acts," published in the *Harvard Theological Review*, January, 1918, pp. 74-99. It is possible that he has modified it since then, as he does not mention it in his later article, but includes (p. 330) both 4:31 and 5:32 in his indications of unity. I will add merely this: On p. 91 he remarks that *the second account is heightened at every point*. Is not this good evidence of unity rather than of diversity of origin? This is the only natural course of events, the way in which the first impact of nascent Christianity against official Judaism must

actually have taken place. First a single incident, the healing of the lame man in the name of Jesus, which with the resulting excitement brought to the authorities the fact of a new and perhaps dangerous teaching. Peter and John were arrested, questioned, and finally threatened and let go. But not long after, the authorities found that the matter was much more serious than they had supposed. The adherents of the new sect had greatly multiplied, undoubted miracles of healing continued to be performed in increasing number, and the people not only of Jerusalem but also of the country round about were excited and inclined to be convinced. The small fire had become a conflagration. *It was plain that more drastic action was necessary, and this was accordingly taken.* Now this all, so far as the main course of events is concerned, has the ring of truth. Nor is the fact to be overlooked that 5:28 ff. takes full and very natural account of the preceding happenings described in chapter 4. Neither chapter could be dispensed with, either in this carefully constructed history or in the actual progress of the Christian beginnings.<sup>1</sup>

Wilson (*Unity*, pp. 329 f.) accepts the date (49 or 50 A.D.) which I had supposed for the Judean document, and strengthens my argument. I cannot feel, to be sure, that the evidence on which I had based my conclusion is "slender." We have here several coincident facts, each one with the obvious possibility of great significance, *and they all point to precisely the same probability.* (1) The author *did not know* (since we certainly must assume that he was a truthful man) the extremely important fact that Silas,

<sup>1</sup>I would like to add also a word of caution at this point in regard to another hypothesis which Wilson proposes along with his suggestion of the doublet account. He is inclined to derive Acts 1:1-11 from Luke 24 (both, of course, in their original Aramaic form); see his interesting argument, pp. 92-99. This derivation seems to me improbable, because the disagreements are much more significant than the agreements. As for the latter, the main incidents of the interval after the resurrection, in their necessary order, were pretty generally agreed upon, we may certainly suppose, at the time to which Luke 24 and Acts 1 both belong. I wish that considerations of space made it possible for me to discuss here the other interesting suggestions made by Wilson in his *Observations*. Perhaps I may have opportunity elsewhere. I will only say here, in passing, that those who have had trouble with the truly difficult passage Acts 12:25 will find in pp. 82-84 a solution which is not only plausible but, to me at least, very probable. I should myself prefer decidedly the Aramaic preposition **בְּ** rather than **בִּי** but either one is possible.

instead of returning with Judas to Jerusalem, remained at Antioch and set out with Paul on a second missionary journey. (2) The following narrative (which immediately corrects his statement) shows that Paul and Silas set out *very soon* on their errand. This is most significant; here is obviously the plausible reason why the narrator did not know what he otherwise must soon have known. Men were going back and forth all the time between Antioch and Jerusalem, and it is at least natural to suppose that the very important question, *how the letter from Jerusalem was received in the Gentile city*, was answered without delay by more than one eager reporter. The one who brought to our narrator or his circle of acquaintance the news of what had taken place must have made the journey to Judea either before Judas (and, as he supposed, Silas also) had set out, or very soon after. If he had waited a few months, or perhaps even weeks, longer, he would have brought a very different report. (3) The Aramaic narrative *comes to an end at just this point*. This was an excellent place for an ending, but by no means the desirable ending place if more events of high importance, in the direct line of the narrator's chief interest, soon followed. Did no one care to know about the further career of Peter and John (both of whom, according to every tradition, went to labor *among the Gentiles*) and the other apostles, or—still more striking and immediately essential—about the dramatic scenes of the return of Paul to Jerusalem? This writer had been greatly interested in the career of Saul of Tarsus, his conversion, and his wonderful work in opposition to Jews and Judaizing Christians; why does he drop him *here*, when his most important labors had just begun?

On the basis of this last fact alone, the ending of the narrative at this point, an unbiased historian, asked to give an opinion as to its probable date, would unquestionably say this: that the presumption, no known fact forbidding, is that it was finished very soon after the event last described, the council at Jerusalem. But when to this fact is added consideration (2), and then the tell-tale misstatement of (1), the result is a very strong combination indeed. Could all this coincidence be accidental? The *one* weighty consideration which will seem to many to forbid accept-

ing my conclusion is the generally accepted view of the origin and date of the Synoptic Gospels. This is no difficulty for me, for I have long been entirely convinced, apart from any investigation of Acts, that these Gospels are of much earlier date than has been supposed, and that they are all three close renderings, without any considerable change, from Semitic originals. Mark I regard as practically a contemporary account. I believe that from the Gospels and from Acts we can get an essentially correct idea of the public career of Jesus and the manner of the earliest spread of Christianity. I mean in the near future to set forth in detail the evidence which has convinced me, and I am not without hope that many others will be convinced. Another "coincidence"—I will number it (4)—which I should add as still further confirming my own view as to the dates of the two halves of Acts is the very natural explanation now possible of the *interval* between Acts and "the former treatise," Luke's Gospel. With the date 50 for I Acts, 60 (when Luke was in Palestine on his two years' visit) for the Gospel, and 64 for II Acts, we have a wonderfully suitable and convincing series. Is this coincidence, again, purely accidental?

Two matters touched upon quite incidentally by Wilson are so important for my own point of view that I am unwilling to let them go without a word of comment. The first is in regard to "slavish" or "illogical" faithfulness of the translator or compiler to the document with which he is dealing. I have spoken already of the matter of close translation, but Wilson's expressed ideas are so typical of those commonly held that it may be well to return to the subject for a moment. Wilson (*Unity*, p. 324) speaks of "the psychological conceivability of such a process of slavishly literal and yet none too accurate translation as is here postulated on the part of Luke." See also page 334, *ibid.* In his *Observations* (p. 83), in speaking of the apparent mistranslation (his own discovery) in Acts 12:25, he says: "To suppose that the present Greek text with *eis* arose from the wrong interpretation of such an original seems to argue an almost incredible stupidity or carelessness on the part of Luke as translator." So, doubtless, very many scholars would say. But we need not waste

time on the psychology of Luke, since his proceedings of this sort are the merest commonplaces in translation-Greek, of which we fortunately have a considerable amount. He renders in just the way (*mutatis mutandis*, since some allowance must be made for the personal equation and for peculiarities of circumstance and immediate aim) in which all the translators of that day performed their task. We need not query whether it is "conceivable," for we have the fact before us in many hundreds of examples, including the work of the best and most learned of the Alexandrine translators, which is saying a good deal. Just such slips as these attributed to Luke are present in all the other renderings, usually in far greater number than here. In both Matthew and Mark they are quite numerous.<sup>1</sup> And do our modern scholars always realize how difficult a matter it is to render a lengthy text without mistakes? I could exhibit renderings made in high-class scientific publications by scholars of note, in this year of grace 1918, which quite throw into the shade any of the blunders attributed to Luke. We must bear in mind also that the whole attitude of mind of the translator then, much more than at present, was that of *close attention to the letter* of the original. In this same connection Wilson expresses surprise at the fidelity of Luke to the *material* of the source he was incorporating. In *Unity*, (p. 334), speaking of the considerations which might have led Luke to alter the "forty days" of Acts 1:3 which contradicts the last chapter of his own Gospel, he says: "To have altered that passage . . . would have impaired the symbolism of the entire opening section of Acts, and Luke's scientific impulses were surely not strong enough for such heroic measures as that." I should say,

<sup>1</sup>The language of the Synoptic Gospels has not yet been adequately examined from the Semitic side. Wellhausen and Dalman have gone but a short distance, leaving the main work still to be done. The whole subject of translation-Greek is to a large extent an unworked field. New and very important light can be thrown on the proceedings of the earliest Christian narrators and compilers in their use of sources. The materials of a complete solution—and the only possible solution—of the Synoptic problem are now at last at hand, as I hope to show in further investigations which are already far advanced and in large part ready for publication. I realize, indeed, that some kinds of evidence can be fully appreciated only by those who have worked laboriously through the mass of material, observing how certain facts and principles demonstrate themselves a hundred times over.

on the contrary, that it was partly because of his scientific impulses that he did not alter it. The “science” of one age is not necessarily that of another, and in *that* age compilers did *not* ordinarily alter documents. Again, page 332, in answering (affirmatively) the question whether Luke or any other person can be conceived to have allowed a misstatement to stand in his source, correcting it himself later on, he says: “The supposition is not an easy one, and yet who shall set logical limits to the things of which the human mind is capable? Certainly no modern scholar would treat a document in such a peculiar way.” But here, again, we are not reduced to the necessity of hypothesis or general probability. The process in question—precisely this—is the ordinary method of oriental historiography. Many of the views which I have been opposing in these pages would not, I think, have been expressed if their supporters had steeped themselves in oriental, and especially Semitic, literature. For we are not dealing with “modern scholars” and methods in these biblical books. All the best Mohammedan historians of the early period, for instance, scholars deserving of the highest respect, habitually put side by side without change the most flatly contradictory accounts of the same events. Whether this method appeals to us or not, it is before us in thousands of very familiar examples. There are numerous well-known instances in the Old Testament; is not one of them the seemingly impossible case of the account of the flood in the days of Noah? Even in the Synoptic Gospels we have illustrations of this truly oriental method. The narrators of that day have at least the appearance of realizing better than we how difficult it is to know with certainty just what happened in a given case; and they were not so cocksure of their own little one-sided interpretations as we often are of ours. The historian might add his own opinion, or leave out whatever material he did not need to use; but to alter deliberately, in the interest of “harmony,” the wording of his source was quite another thing—and a *very unusual thing.*<sup>1</sup>

<sup>1</sup>I have long been convinced that nine-tenths, at least, of the “tendency alterations” seen by modern commentators in the books of the Old and New Testaments are not such in reality.

The other matter to which I have referred is a misconception less illustrated than suggested by words which Wilson uses. He speaks (*Unity*, p. 331 f.) of the accounts of Pentecost, Paul's conversion, and the incident of Cornelius, as "thickly encrusted with legend," and the inference from the context is that they are therefore not contemporary accounts, but productions of a later day. This is a view very generally held, and my protest against it is again based on the study of ancient oriental peoples and documents. The mere fact of (to us) incredible *details* does not even make it probable that the account is late, to say nothing of any necessity in the case. It is more than fruitless to conjecture "how long a time would be required" for this or that story of divine intervention to grow up; even in modern Jerusalem, or Hebron, or Jaffa, they could easily, and do, make their appearance overnight; they have been matters of course there from time immemorial, but are especially characteristic of the ancient time. In the best of the histories and biographies which have come down to us from this early age and this part of the world we have a solid core of fact overlaid with what Wilson rightly terms "embellishment"—and the embellishment, or rather its material, is generally older than the main account! Part of it is artistic, but the most of it belongs to the psychological background. We must of course bear in mind two things: first, that these records were composed in good faith by able writers who certainly were not far removed from the times and scenes described; and second, that while the people are not mistaken in the long run as to the *general course* of events in their day, there is very likely to be great uncertainty and a conflict of reports as to *details*, and the latter require only the briefest time for making their appearance. Students of the earliest Christian records will do well, as I have said before, to study the oldest lives of Saint Simeon Stylites, which we are fortunately able to date accurately, and they will see how just such "embellishment" as we are here considering arose, and was generally believed, during the lifetime of the saint, although the *main account of his life* is in each case a true one. Yet these histories of the saint, I should say, have by no means the same high proportion of trustworthiness as our narratives in

the Gospels and Acts. How soon after the events which it describes can the Gospel of Mark have been written? There is very little in it which, *judging from the evidence in hand which we can control*, might not have been written down during the lifetime of Jesus, and nothing at all which might not easily have been composed within a few years after the resurrection, since there is in it no evident allusion to, or building upon, later circumstances or events.

To return to the special incidents in Acts singled out by Wilson. His conclusion as to the happening on the day of Pentecost is the same to which I myself had come. There must have been a time when the phenomenon of "speaking with tongues" first appeared in the Christian community. No circumstances of which we have knowledge could have been so likely to give rise to this manifestation of spiritual ecstasy as the scene in the upper room, when the disciples were waiting with splendid faith for *something*—they knew not what—and the *Ruach Elohim* seemed to come in like a rushing wind. The rest is interpretation, and quite inevitable. Upon the question whether the explanation of the phenomenon as utterance in foreign languages preceded or followed the true explanation, the account throws no light whatever; for we may be very sure that the rank and file of the Church would never have placed the experience of the Twelve Apostles on this most momentous occasion on a par with and equivalent to the experience of the ordinary believer, which was almost immediately seen, we may suppose, to have nothing to do with foreign tongues, and was rated low enough by Paul, who however thanked God (I Cor. 14:18) that he was more expert than any of his hearers in exercising this curious gift. In any case, the glossolalia of the *Twelve* meant in the belief of the church, almost or quite from the first, the proclamation of the new truth in the languages of the Gentiles.<sup>1</sup> Why may not the story of the Pentecostal

<sup>1</sup>We should naturally suppose that the *first* explanation of the glossolalia would have been that of speech in foreign languages; not merely because the idea of proclaiming the gospel to the brethren living in other lands must have been constantly in the mind of the disciples from the very first, but still more because of the impression which the phenomenon in its earliest appearances would inevitably produce. This is the way the speech of a foreigner sounds to those who hear without understanding it. There were once brought to me in New Haven phonographic records

awakening be contemporary, and of high value for us? I can see no reason. The same can be said of the account of Paul's conversion in Acts 9,<sup>1</sup> and of the story of Cornelius. I believe that I Acts is a document whose value, from beginning to end, will be prized more highly as its nature is better understood. As in the similar literature of which I have spoken, we have two main elements: the broad outlines, which are solid and true, belonging to the world for all time; and the incidents, the details, which are essential to the mental habit of the time and place. Seekers after historical verity will of course interpose here that we cannot ordinarily draw a sure line between the fact and the fiction, and that we are therefore without a trustworthy picture of the successive events. Quite true; we could not in any case have such a picture, and we do not need it; what we have is better. If by some miracle there could have been produced, instead of our Gospels and Acts, a "historical" account of the bare events—that which often seems to us so desirable—we should be much worse off than we are at present. As it is, we are sufficiently provided with the essential facts, and have in addition what is more important still, the materials which enable us to get somewhere near the heart of the people themselves, learning something of their point of view and the background of their theological and religious conceptions. Without these "embellishments" which so often distress us, both life and truth would be gone from the record.

I ought to add, that Wilson's own view of these matters seems to be substantially the same as mine. In regard to the Cornelius incident, for example, he says (p. 331): "There is no reason to doubt the essential fact," and he expresses himself similarly in many other places.

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of the utterances of an uneducated young girl in a Maine town, who "spoke with tongues" in religious ecstasy. It was believed by many, I was told, that the speech was Hebrew; on this I was to give judgment. The records were very interesting to me; it seemed a clear case of "tongues" like those of the Apostolic age; but it is perhaps needless to say that the "language" was not Hebrew, nor any other with which I am familiar.

<sup>1</sup> Wilson's remarks (*Observations*, pp. 84-89) on the three accounts of Paul's conversion seem to me admirable.

The one long and detailed review of my *Composition and Date of Acts*, by Professor Bacon in this *Journal*,<sup>1</sup> deserves some further answer here. While agreeing with me in many matters, in others he sharply attacks both conclusions and principles, aiming the faithful blows of a friend in most vigorous fashion. I hope to give an equally frank and friendly rejoinder.

Bacon accepts at the outset, as I have said, my main conclusions as to the language of the two parts of the book, but remarks (p. 4, note 3): “Allowance should be made for some degree of overstatement as to the absence of Semitisms from II Acts (p. 7) and absence of revision by the Greek editor from I Acts.” I plead not guilty to the charge, and would like to see the evidence. Bacon has *one* instance, proceeding: “e.g., in 1:18–20. (Note τὴν ἐπισκοπήν in vs. 20.)” Sure enough. Any one who is so fortunate as to have a Greek Old Testament within reach can readily satisfy himself that the quotations in vs. 20 are taken directly, as usual, from the LXX of Ps. 69:26 and 109:8, and that in the latter passage in particular the citation is exact, including the τὴν ἐπισκοπήν!<sup>2</sup> Until something better than this is produced, then, I shall continue to cherish the belief that my statement was a fair one.<sup>3</sup>

On pages 8f. three “historical” objections—“egregious anachronisms”—are opposed to my suggested date for I Acts, all three apparently put forward by Bacon with considerable confidence, and all three quite worthless for the purpose in hand. The Theudas-Judas problem I had mentioned in my work (p. 71), remarking—all that it is possible to say with any certainty—that the mixing of dates had its origin in second-hand information misunderstood, and that the source of it was *not* Josephus. The only question to be raised here is whether it is possible that the original account

<sup>1</sup> January, 1918, pp. 1–23.

<sup>2</sup> Was Bacon misled by the familiar words of the *English* (Authorized) version: “And his *bishopric* [!] let another take”?

<sup>3</sup> As for II Acts, if Bacon will take another look at my pamphlet, p. 7, he will see that I merely said that “there is no evidence of an underlying Semitic language.” The “Semitisms” which I happened to mention are only typical; more could easily be added, and, as I remarked, there are different ways in which they can be accounted for.

should have been written, or the transfer of information otherwise made, before the year 50. Of course it is possible. The thing might have happened at any time after, even immediately after, the execution of the sons of Judas. It is easy to think of a score of ways in which the mistake might have occurred, whether we suppose a formal historical document or not. For instance: A writes to his acquaintance B, who has joined the rebellious sect called Christians: "Better get out of it while you can. It will not do to resist the authorities and follow upstart leaders. Remember Theudas, who boasted . . . etc. And now again see what has happened to the sons of that misguided and ill-fated man, Judas of Galilee, who uprose in the days of the census, . . . etc." B had never heard of Theudas, which is not in the least surprising,<sup>1</sup> but from the order of mention formed the very natural but too hasty conclusion that he preceded Judas of Galilee. He had no intention of "getting out" of the persecuted Christian brotherhood, and saw instantly how the comparison of the uprisings under Theudas and Judas could be used as a literary embellishment in the historical work (our I Acts) which he was writing. Why not? It is as legitimate to exercise the imagination in favor of our writer as to use it against him. He may well have been one of the fugitives from Jerusalem in the year 44, or a resident of some other city of Judea. Putting the allusion to these two insurgents into the mouth of Gamaliel proved to be an unfortunate literary touch, to be sure, both because the surmise as to the date of Theudas was mistaken, and also because the account would have been better off, as literature, without this addition. It may possibly have been an afterthought on the part of our author, or even an improvement inserted by some later hand. Gamaliel's speech is decidedly more forcible when this illustrative parenthesis is omitted.

<sup>1</sup>We need to remind ourselves that at the time of this Theudas incident, in the year 44 or 45, the church had just been undergoing one of its first severe persecutions, the one in which James the son of Zebedee met his death. The Christians of Jerusalem had been obliged to flee for their lives, and had other things to attend to than such trivial matters as the brief disturbance made by Theudas and his four hundred followers. Bacon argues (p. 8, below the middle) that if our author did not know of Theudas he cannot have been acquainted with any of the far greater happenings ("these recent events") of the years 44-49—a thesis which it would be rather difficult to maintain.

thesis (vss. 36–38a) is left out. Our otherwise high opinion of the literary skill of this writer would be maintained here if we could suppose that in the text as he originally wrote it verse 38b followed immediately upon verse 35. The two examples are not very impressive in themselves, nor are they as well handled as we should expect.

The second argument, equally time-worn—though hardly so in the form given it by Bacon—concerns the mention of the centurion Cornelius and the “Italian cohort” in chapter 10. He expresses himself as follows (pp. 8 f.):

In adopting the date 49–50 for the Aramaic Document we shall also be compelled to suppose that the Roman garrison, established after the suppression of this revolt of 45–46 in Caesarea, the capital of the province, had long been resident there during the reign of Agrippa and that its commandant had even endeared himself to the whole Jewish population. . . . Or can it be that the author of I Acts did not remember that the rule of the procurators with its “Italic cohort,” stationed in Caesarea did not begin till after the death of Agrippa?

These are amazing misstatements. Either Professor Bacon has not himself consulted the original sources of our knowledge concerning these matters or else his memory has served him very ill. In the first place, we have no information whatever, aside from Acts 10:1, as to the presence of an Italian cohort in Caesarea *at any time*. It is a mistake to suppose that we hear of one “stationed” there under the procurators. Then as to our being “compelled to suppose” that the Roman garrison had been in Caesarea for some time previous to the year 45: is it possible that Bacon is unaware that just this, the idea of which calls forth his sarcasm in the footnote to page 8, was true? It is one of the few things which we happen to know definitely and with certainty about the Caesarea of this period. If he will consult Josephus *Antt. xix. 9. 2* he will see it expressly stated that the Roman cohorts (*σπεῖραι*) which were in Caesarea in the days of Gessius Florus (66 A.D.) were the very same that had been there in the time of Herod Agrippa, before the rule of the procurators! Josephus tells us how Claudius, in sending Cuspius Fadus to Judea on the death of Agrippa, gave him the order to remove to Pontus

the troops stationed in Caesarea and Sebaste and replace them by others; but then goes on to tell how the order was speedily countermanded, so that the cohorts were *not* transferred (*οὐ . . . μετέστησαν*), but remained just as they were and ultimately became one chief cause of the troubles under Florus<sup>1</sup>. It has even been shown to be probable, and is, I suppose, generally believed, that practically this same Roman garrison had been in Caesarea for a long time before the reign of Agrippa.<sup>2</sup>

Bacon says (*ibid.*): "It seems really a pity that Agrippa should not have known of the presence of this amiable officer when, shortly after (Acts 12:19-23), he came down to Caesarea threatening war against 'them of Tyre and Sidon'"; implying that if there had been Roman troops there they would certainly have been called out on this occasion and have been mentioned in the account given in Acts. Anyone who will read Josephus, however, can see that Agrippa and these Roman troops of Caesarea were well acquainted with each other, though the king was disliked by the soldiers because of his partiality for the Jews.<sup>3</sup>

Some excellent scholars have indeed doubted the statement of Acts 10:1 that one of these cohorts in Caesarea was *Italian*; so especially Schürer, *Geschichte*, I, 462 f. We happen to know, from inscriptions, of at least one "Italian cohort" in Syria between the years 69 and 157;<sup>4</sup> but the presence of one in Judea, in the time of Agrippa (41-44 A.D.) or earlier, has been questioned for two chief reasons: first, and most weighty, *the postulate of a late date for Acts*; and second, the reflection that a band of Italian troops would not have been likely to serve under a Jewish king. This latter reason is of very little weight. We are not dealing with modern Europe, nor even with an independent Jewish kingdom, for Judea was completely under Roman control. It may well be that the very *cohors Italica* about which we have information a few years later was stationed in Caesarea at this time.

<sup>1</sup> Cf. *Bell. Jud.* ii. 12. 5; 14. 4-6; 15. 3.

<sup>2</sup> Schürer, *Geschichte des jüdischen Volkes*<sup>3</sup>, I, 460 f.

<sup>3</sup> There is indeed a "bad break" here (see Bacon's footnote, p. 9), but it does not fall to the account of the author of I Acts.

<sup>4</sup> See Schürer, *ibid.*, note 53.

There was certainly no city in all Palestine where it would have been so likely to be located at first.

From the time of the Emperor Augustus onward, the "auxiliary" troops recruited in the provinces were reinforced more and more by bands of native Italian volunteers. The reason for this was not primarily love of adventure, but the fact that in military service, now a dependable trade, the cohort offered lighter labor and more attractive conditions than the legion. The famous military historian Flavius Vegetius<sup>1</sup> states this with some emphasis, and tells us that there was a rush to enlist for service in foreign lands; that is, in the cohorts of the auxiliary army. He says (II, 3) in regard to the legions: *Magnus in illis labor est militandi, graviora arma, plura munera, severior disciplina. Quod vitantes plerique, in auxiliis festinant militiae sacramenta percipere, ubi et minor sudor et maturiora sunt praemia.* These were the conditions in general. As for Caesarea in particular, it was a new city, built for the Roman emperor, in the Roman style, and in the interest of Roman customs and civilization. It had a climate like that of central Italy. Herod governed for the Romans a part of their empire, and was eager to make the connection with the motherland as direct and strong as possible. This was true also of his sons, and indeed of the whole Herodian family. Augustus, for his part, was especially concerned to establish order and security in this rather troublesome province; he may also have thought it desirable to encourage the Romanization of the new capital in every practicable way. At all events, he or his advisers could hardly have done a wiser thing than to include an Italian cohort among the native Syrian auxiliaries, stationing it in this city and putting in command of it a man of just the qualities of Cornelius.

During the period with which we are concerned there were five cohorts of foot-soldiers in Caesarea.<sup>2</sup> As usual, they were mainly troops recruited in the region where they were to serve, the bulk of them in this case being men of Sebaste and Caesarea, as Josephus informs us. *A portion, however, came from outside*

<sup>1</sup>Quoted in Marquardt, *Römische Staatsverwaltung*, II, 453.

<sup>2</sup>So continuously, it seems, from 4 B.C. down to the time of Vespasian; see Schürer, *op. cit.*, p. 461.

Syria (*Bell. Jud.* ii. 13. 7); Josephus is not concerned to tell us the origin of these. The troops of Sebaste and Caesarea, we are told, were hostile to the Jews, and also ill-disposed toward the house of Herod. They celebrated in a most unseemly manner the death of Agrippa, although he had dealt generously with them (*Annt.* xix. 9. 1). On the occasion of the conflict between the Syrians and Jews of Caesarea in the time of Felix, the Syrian soldiers aided their fellow-countrymen, while the part of the garrison that was not Syrian seems to have kept out of the disturbance (*Bell. Jud.* ii. 13. 7). When the troops of Caesarea were called out to suppress the serious Jewish uprising of the year 52, described in *Annt.* xx. 6. 1, it may be remarked that *four* of the five cohorts of foot-soldiers were employed. It is at least a reasonable supposition that the remaining cohort was the *σπεῖρα Ἰταλική* of Acts 10:1, and that it was deemed politic to keep Cornelius and his Italian soldiers out of these local quarrels as far as practicable.

The third "egregious anachronism" is in regard to the date of the famine in Judea mentioned in Acts 11:27-30 and 12:25. Bacon, page 9, speaks of "the frightful famine of 46-48," and wonders how a contemporary narrator can have forgotten that it "was not before the persecution and death of Agrippa (11:27-30), but at least a year or two after it." The representation in Acts, however, as scholars generally have understood it, is precisely the contrary of what Bacon affirms! The *prophecy* took place before the persecution under Agrippa, indeed before the year 41, but the *fulfilment* came several years later. The position of the brief allusion to the latter in 12:25, *after* the account of the death of Herod Agrippa, makes the thing plain enough.<sup>1</sup>

After this we are quite prepared to read in Bacon's article, page 9, that these "mistakes" on the part of the author of I Acts "are mere examples from a whole series of corroborative evidences." Where "egregious anachronisms" are being manufactured in this

<sup>1</sup>The date, 46-48, which Bacon assigns to the famine looks like a slip of the pen. Jos. *Annt.* xx. 5. 2 tells us that the famine began in the time of Cuspius Fadus and continued into that of his successor Tiberius Alexander; see Schürer, *Gesch.* I, 567, note 8. The date of the transition is not exactly known, but was probably the year 45. The famine may be dated 44-45, or 45-46 at the latest.

untrammeled way, there is no need to stop at three, or ten, or a score.

It is not surprising that Bacon should insist, with considerable use of italics (p. 9, below), that for theological reasons it is necessary to suppose that Paul's view of the resurrection and the immediately succeeding events (which view Bacon first interprets arbitrarily and then styles "the Apostolic tradition") antedates the general view contained in the Gospels; whereas it seems to me quite plain that every particle of literary and theological probability points in the other direction. The assumption is essential to Bacon's general position, and I have sufficiently expressed, above, my own view as to what is likely to constitute primitive popular tradition. It is on the other hand unexpected to see him using as arguments the careless assumptions which are becoming too familiar, handed from one to another, such "securities" as may be had from any curbstone operator in theological speculations, but are not to be looked for in the possession of a scholar of Bacon's rank. Thus, page 9: "A date in the later years of Domitian, when expressions such as 'suffering for the Name' (Acts 5:41) had begun to obtain currency." Where, I would ask, does he get his information that the phrase "began to obtain currency" in the later years of Domitian? Certainly not from general probability, for on that ground we should expect that both the fact and the characteristic Jewish form of words used to express it would have appeared within a very short time after the death of Jesus. Certainly not from any direct testimony; nor from any body of early Christian literature which could afford a basis for judgment. On the contrary, the assertion which I have quoted is merely a begging of the question.

As I have said, Bacon is far from believing in the unity of I Acts, and is especially sure that chapters 13 and 14 originally belonged elsewhere. He asks (p. 6) how we can explain the "truly marvelous coincidence" that the story told in these two chapters is continued in chapters 16 ff. My own explanation of this is a simple one. I believe, first, that the account of events given by the two writers is substantially *true* and not a mere invention; and second, that the translator of I Acts, being, obviously,

the same man who composed the Greek of chapters 16 ff., naturally continued where his source had left off. It is the same "marvelous coincidence," then, which we have in any book in which chapter 2 goes on from just the point where chapter 1 had finished. "What ground," Bacon asks (*ibid.*), "have we for imagining this happy and romantic discovery" of the Aramaic document? (The discovery was certainly a "happy" one, though not surprising; Bacon's *Autor ad Theophilum*, he tells us [p. 11], "was really in search of such documents," and doubtless many were to be had in Palestine at the date I have supposed; the "romance" I leave, here as elsewhere, to my colleague.) I will try to answer his question. We have precisely the same ground that we have for believing that the Chronicler of the Old Testament made the happy find of an Aramaic document which he transcribed *and continued* (as no one doubts) in his Book of Ezra, or that he made a similar find *and continuation* in his Book of Nehemiah. We see him doing these things before our very eyes—just as we see them done in the Book of Acts. Bacon remarks incidentally (p. 7) that "Acts," Preaching, and Travels, of apostles (i.e., what we have in Acts 13 and 14) are "a *Greek* type of literature," and not an Aramaic type. How in the world does he know this? He rejects with some asperity (p. 16) my expressed opinion that if the hypothesis of translation of I Acts is accepted it is not likely that any convincing theory of its composition will be put forth. As to this, I appeal, first, to his own remark (or is it that of another author? I suspect composition) on the next page, to the effect that my demonstration of translation "deals the *coup de grace*" to Harnack's attempt (in I Acts) to build theories of composition and date on mere phenomena of vocabulary and style. Some ground, then (and it is ordinarily the *only* sort of evidence that has a chance of being generally convincing in such cases), has been taken from under those who would find "A, B, and C sources" in I Acts. How did the case stand before this? I turn next to what Bacon (or rather, the other author) says on page 16, near the top: "Probably there is not a single advocate of the theory of composite origin who would not say the same," viz., that "he has been convinced by none" of the theories put forward by others

than himself. "Each finds enough of error in the method of his fellow-workers to reject much of their individual results, and enough of truth to corroborate, strengthen, and enlarge his own." It would seem, then, that my statement may stand as I made it. I certainly have not expected "to relieve [those who are in search of mares' nests] from all further trouble." It is easy to "analyze" any document, if one is equipped with a microscope, a magnifying glass, and an eye trained to look for discrepancies; and I have little doubt that there will be scholars not a few who will see in Acts separate Peter, Paul, Philip, Stephen, Barnabas, and Silas documents, besides Caesarean, Ephesian, Roman, and other sources, all united by that *deus ex machina*, "the redactor." Bacon recognizes, indeed, an underlying and well-conceived plan, however obtained, in I Acts, and approves (p. 21) my statement of it—which he attributes to Harnack.

There appears to be a wish to make a curious distinction, even in the title of the article (see also pp. 14, 16, 18 ff.) between "philological criticism"—which in the further discussion seems to mean ascertaining facts—and "historical criticism"—which seems to mean dispensing with them. Of course what we are all striving after, in these researches of ours, of whatever nature, is the *history*, and I am far from wishing to seem unconscious of all the valuable work which my colleague Bacon has done and is doing in this most important field. But is not the term "historico-critical" sometimes used in these pages where it is not accurately descriptive of the real process? I am taken to task (pp. 14, 16, 18, 20) for paying so little attention in my pamphlet to the work of some foreign scholars. I had at least the wish to be fair to all; but having reached, as it seemed to me, totally new standing-ground, I found my point of view so different from, and irreconcilable with, that of the investigations referred to, that any attempt to argue with them in detail would be a waste of time at present. I felt it to be best to state my own conclusions as briefly and clearly as I could. Then, when one competent scholar after another sees and sets forth (more sharply than I could) the precise points at which his view clashes with mine, as Bacon has done in his review, I can attempt, as I am doing here, to defend my

opinion. In the very few cases where conclusions recently published and germane to my argument had gained, or seemed likely to gain, considerable acceptance, I took account of them, as e.g., Norden's discussion of Acts 17. I read carefully through Wellhausen's "Noten zur Apostelgeschichte" and his "Kritische Analyse" without finding in either one of them a single new idea which I could regard as useful. How it was possible for him to study the Greek of the first half of the book again and again without ever seeing the evidences of translation, is hard for me to understand. Krenkel's comparison of Josephus is a thick book which requires either a very long (and extremely thin) discussion or else only a few words. How many zeros does it take to make a finite quantity? Harnack's recent work in this field might seem to demand more immediate attention. Wilson, *Observations*, page 90, speaks appreciatively of Harnack's work *as compared with that of the other disectors of the book*; but on the next page remarks that his "more pretentious efforts issue, by his own confession, in little more than general probability"; and on page 99 he speaks of "those elaborate reconstructions such as Professor Harnack has built up only to decide that they are too shaky for permanent habitations."<sup>1</sup> Theories which fall of their own weight when even their own author looks at them critically certainly do not *require* to be treated in a small pamphlet like mine. The fact is, there is no portion of the Bible which has offered such an opportunity for wild guessing as the Book of Acts. On a basis of conjecture other conjectures are built up into a high and complex structure, by a method which one is tempted to call "aërial criticism," since it has no point of contact with solid earth. A favorite way of laying the foundation is indicated by Bacon on page 16: "The task of comparing the point of view of the Autor ad Theo-

<sup>1</sup> This is just the impression which Harnack's recent investigations in Acts have made upon me also, that the most of his conclusions are neither sound nor useful. Like the other workers in this particular field, he found himself reduced to mere conjecture, in the *impasse* to which the study of the book had come partly because of the supposed necessity of late dates, but still more because of the bewildering fact that diverse authors seemed to be writing in the same well-marked language and style! But when he proceeds to designate a series of oral sources (such as the testimony of the daughters of Philip) he abandons, as Wilson, p. 90, justly says, all genuine literary analysis.

philum with the often apparently quite different point of view of his sources"—that is, comparing things about which we cannot possibly, in the nature of the case, have even approximately satisfactory knowledge. We are told, on page 20, "precisely" the doctrinal standpoint of the Autor ad Theophilum; and another would tell us, on perhaps equally good ground, that it is "precisely" the opposite.<sup>1</sup> And it is on the basis of what is assumed here that the book is made a scrap-basket and its writers discredited. This "aërial" work is generally interesting and occasionally useful, but it never has any important bearing on historical research.

Bacon derives from my demonstrations of translation one or two general principles which, if they could be allowed, would be veritable bombshells—everywhere. The first of these, stated on pages 17 f., is to the following effect. Since it is shown that the *translator* of a document uses, in the rendering, his own vocabulary and peculiar habits of speech and shows traces of his own literary style, therefore (!!) the *transcriber* and *editor* of a document in his own language presumably rewrites it in his own form of words. The other principle results naturally from this (p. 18): Mere similarity of style and vocabulary counts for little or nothing as an evidence of homogeneity. Nothing of the sort is true, however, in either case, but precisely the contrary. Both "principles" have indeed occasionally been adopted, in sheer desperation, especially in the "aërial criticism" of the Old Testament, but also in the New; see for example the words of Bousset quoted by Bacon (p. 18, note 1). The fact, perfectly demonstrable and in accord with every probability, is that the translator uses, of course, his own language; the transcriber or editor retains the wording of his document with the least possible change, generally with no change at all. I know of no exception to this rule in

<sup>1</sup> As for the author of I Acts, we do not know whether he was Jew, or Gentile, or proselyte; whether he wrote in Jerusalem, or Hebron, or Gaza, or Antioch, or Caesarea; for what readers he wrote, or with what immediate purpose; whether his "doctrine" was of this type or that (seeing that he writes objectively). It seems plain that he had spent the most of his life in Judea; and it appears to be with satisfaction that he narrates the complete discomfiture at Jerusalem of the "Judaizers" who came from Judea to Antioch in the interest of the Mosaic law.

either Old Testament or New.<sup>1</sup> It is also the rule in the oriental profane literature, of whatever sort or language. *Wherever*, in the Gospel or Acts, Luke's own vocabulary and style appear, Luke is either translating or composing freely. And if in the Gospel, for instance, Luke's own characteristic forms of speech are seen to be mingled with unmistakable material from Mark or Matthew, the explanation, barring the possibility of later harmonizing hands, is that his own rendering was colored by sight or memory of the other Gospels.<sup>2</sup> Language and style have always been, and will always continue to be, among the very best of criteria. A man may easily change his opinions, or his chief interests, from time to time; his language and literary habits are not so readily altered or concealed.

The hypothesis that the two demonstrable sources in Acts are the *only* documents in the book will, I believe, be found to satisfy all requirements save those of theologico-conjectural (sometimes termed historico-critical) research. It will not be easy to find good ground for going behind them. I cannot imagine where Bacon has found the "mental inertia" (p. 6) which is said to lead scholars to assume the priority of Semitic documents over Greek, in any case where the matter could be in doubt; certainly not among the Semitists, for they could save themselves a good deal of trouble and responsibility by supposing the Greek to be the original. It is of course simply a matter of evidence, in each and every case. Nor have I ever heard of "the common assumption of priority of Aramaic over Greek in early Christian sources" (p. 7) except to this extent, that the background of the earliest tradition is, admittedly, Aramaic, and we ordinarily expect to see water flow from the spring into the stream rather than vice versa. Bacon thinks (p. 22) of a possible attempt to find a Greek

<sup>1</sup> Even that arch-refashioner of tradition, the Chronicler of the Old Testament, rarely makes verbal changes in his sources, but writes out in page after page and chapter after chapter just what he finds before him. When he wishes to introduce his own views he adds single sentences, or paragraphs, or whole chapters. Of course he omits whatever he does not need.

<sup>2</sup> I should slightly modify now some things which I said in my *Translations Made from the Original Aramaic Gospels*. I was not then so sure as I am now that Luke used *only Semitic sources* in compiling his Gospel.

source *back of the Aramaic* in chapters 13–15 of Acts; but the experience of the one who should make the attempt would certainly be that which is described in someone's definition of the science of metaphysics: a blindfolded man in a dark room, trying to get hold of a black cat—which isn't there. I can hardly think that the suggestion (p. 23) that the quotation in 15:16–18 gives evidence of an original *Greek* document, behind the Aramaic, is meant seriously. Where, Bacon asks, is this form of verse 17 found? and he answers, “*Only in the LXX.*” But in the very next sentence he (or the other author?) contradicts this statement flatly and finally by showing, beyond all question, that this precise form was *in the Hebrew* which the Greek rendered. He certainly would not be so rash as to attempt to prescribe the geographical limits within which this form of the Hebrew text circulated, or the extent of its popularity. It probably had been familiar for centuries, in all parts of the Jewish world. A multitude of just such variants, as we well know, were current in Judea and Jerusalem; many of them found their way, as *qerē* and *kethibh*, even into the text which was made official more than half a century later than this. The very fact of these much-used variants was the chief reason why a “standard” had to be adopted at last. The proof-text in this form is particularly attractive here, as Bacon and others have remarked; is it difficult to suppose that the Aramaic author was intelligent enough to see this?

This is quite sufficient in reply; but I myself should not stop here. To me, it is a certainty that the Hebrew text just as we have it in our Book of Amos, 9:11 f., fully meets the need of this important passage. By “the remnant of Edom” is meant (as I believe parallel passages show conclusively) those from the hostile nations of the world—“Edom” is a standing designation of them—who shall survive the great catastrophe ushering in the messianic age. The God of Israel keeps them for himself, his name is put upon them. As we are told in Isa. 25, he will take away the veil that is now over all the foreign peoples, and will wipe away all tears from their eyes; and in Isa. 66 it is said that he will even take from them some to be his priests and Levites. The church of the Messiah “will gain possession of them” (Amos

9:12). Nothing more than this could possibly be required for the passage in Acts; though, as I have said, the other form of the Hebrew is more striking.<sup>1</sup>

I trust I have not seemed, in the preceding pages, to claim for myself a monopoly of fact, leaving all the fancy to those who hold views differing from my own. On the other hand, I believe I have shown that certain conjectures which form the basis of widespread views of the composition and date of Acts are less plausible now than they were formerly. New material, of truly fundamental importance, has come to light, and it will make necessary a revision of long-accepted theories. The task calls for specialists in more than one field, but the work of sifting and co-ordinating will of course belong especially to the expert New Testament scholars, such as my opponent for the nonce, Professor Bacon. And may all this debating, with its necessarily emphatic *sic et non*, bring us nearer to a true understanding of these writings which are among the most important of all time!

<sup>1</sup>The passage in Amos is one of a great many, intimately connected in their underlying thought, which seem to give a choice between two interpretations: the one broad and noble, worthy of the great Hebrew seers; the other narrow and vindictive, worthy—I am tempted to say, of a type of Old Testament exegesis which is all too common at present.